valuable, from its containing a large number of measures of double stars made by Mr. S. W. Burnham during his temporary connection with the Observatory, from April 23 to September 30, 1881. The measures are contained in three catalogues: (1) a list of sixty new double stars discovered in the zone observations, chiefly by Prof. Holden; (2) a list of eighty-eight new double stars discovered and measured by Mr. Burnham; and (3) measures by the same eminent observer of 150 double stars from his manuscript general catalogue. A number of difficult objects are included in the third series. δ Sextantis was not elongated with the highest powers at the epoch 1881·34, nor did γ Coronæ show any sign of duplicity; as Mr. Burnham remarks, "it has been apparently single with all apertures since about 1871." Among the more difficult binary stars there are measures of O.Σ. 235, Σ 3123, 42 Comæ, Σ 2173, β Delphini, δ Equulei, and 85 Pegasi. There are also positions and descriptions of eighty-four red stars, of which twenty-seven are stated to be new, and a list of new nebulæ and clusters discovered in the zone-observations at the Washburn observatory.

The position of the observatory is in latitude 43° 4′ 36″·6 N., and longitude 89° 24′ 28″·3 west of Greenwich. Prof. Watson, of Ann Arbor, Michigan, was first appointed to the superintendence, but at his premature death in November, 1880, he had not been able to commence astronomical observations. Prof. Holden gives some account of the preparations he was making for scientific activity, as the only way of associating his name with the observatory.

CHEMICAL NOTES

HERR LELLMANN (*Berichte*, xv. 2835) describes an interesting case of *physical isomerism*. Dibenzyl-diamido-dibromdiphenyl melts at 195°; if the liquid so produced is quickly cooled, the solid now melts at 99°, but on heating again solidifies at 125° to 130°, and melts a second time at 195°; if the substance melting at 195° be slowly cooled and then again heated, the melting point now observed is 195°.

BERTHELOT AND VIELLE (*Compt. rend.* xcv. 129) sum up the results of their researches regarding explosive waves. The propagation of an explosive wave occurs when the ignited stratum of gas exerts the maximum pressure on the adjacent stratum; increase of pressure is accompanied by increased velocity of propagation. To produce an explosive wave it is necessary that a considerable mass of gas should be employed and that the cooling by radiation and conduction should not be great; if the temperature fall below 1700°-2000°, or if the volume of the products of combustion is less than one-fourth, or in some cases one-third of the total volume of the final mixture, the propagation of the wave ceases.

ACCORDING to the experiments of M. Corne (*J. Pharm. Chim.*, [5] vi. 17) the glowing of phosphorus is due to volatilisation of the phosphorus and subsequent production of ozone by electrical energy generated by the volatilisation of the phosphorus. Phosphorus does not glow in pure oxygen at high pressures because, says M. Corne, volatilisation is impeded and at a certain limit becomes too slow to ozonise the oxygen. Gases which hinder the formation of ozone also prevent phosphorescence.

BAEYER (*Berichte*, xv. 2856) has obtained nearly pure indigo blue by acting on a solution in acetone of ortho-nitrobenzaldehyde. Acetone and nitrobenzaldehyde react to form a condensation product, $C_{10}H_{11}NO_4$, from which alkali withdraws the elements of acetic acid with production of indigo-blue, thus—

$$2C_{10}H_{11}NO_4 + 2H_2O = C_{16}H_{10}N_2O_2 + 2C_2H_4O_2 + 4H_2O.$$

J. HORBACZEWSKI (*Berichte*, xv. 2678) has obtained uric acid by heating together glycocoll and urea to 200°-230°; details of the reaction are promised.

THE working of the Food Adulteration Act for the year 1881 is considered in a Report of the Local Government Board lately issued in a Blue Book, and published in *The Analyst* (vii. 218). The total number of districts in which analysts were acting at the close of December 1881, was 260; during the year 17,823 samples were analysed, of which 2613, equal to 14·7 per cent. were reported as adulterated; in 1877, 14,706 samples were analysed, and 19·2 per cent. reported as adulterated. More than a third of the samples analysed, and more than a half of those reported against, were of milk. Birmingham still "maintains the distinction which it has for some years enjoyed, of having a larger proportion of its milk reported as adulterated than any other great town in the kingdom." The adulteration of bread and of butter seems to be steadily on the decrease; in coffee the proportion of adulteration is rather less than last year; chicory is still the commonly used adulterant. The adulteration of sugar is practically a thing of the past. More than one-fourth of all the samples of spirits examined were reported as adulterated, chiefly with water: a good deal of gin is sold containing not much more than 20 per cent. of alcohol.

PROF. HOFMANN describes in the *Berichte* (xv. 2656) a number of interesting lecture experiments. To determine that no loss of matter occurs during combustion, he employs a two-litre flask fitted with a cork carrying a small manometer, a glass tube with stopcock, and a straight piece of rather wide tubing to the under-end of which a small porcelain crucible is attached. The wide tube is closed by a cork, about half a gram of dry phosphorus is placed in the little crucible, a portion of the air in the flask is pumped out, and the flask—and also a little bit of stout copper wire—is counterpoised; the little bit of copper wire is heated and dropped down the wide tube, from which the cork is withdrawn for a moment; the phosphorus is thus ignited; after the combustion, which proceeds slowly, is completed, the flask is found to weigh the same as before; the stopcock is now opened, air rushes in, and the flask now weighs more than it did at the beginning of the experiment.

To illustrate the great difference between the volumes of equal weights of liquid and gaseous water, Dr. Hofmann employs a glass bulb of about 300 c.c. capacity, with a narrow glass tube at each end, the upper tube being fitted with a stopcock. This apparatus is supported so that the lower tube reaches to about 1 centim. from the surface of the mercury in a basin; a rapid current of steam is passed into the apparatus; after five minutes or so, when every trace of air is expelled, the stopcock is closed, and at the same moment the lower tube is pushed beneath the mercury, which at once begins to rise into the bulb; after a little time the bulb is almost filled with mercury, on the surface of which the condensed water appears as a thin layer.

VERY simple apparatuses are also described for containing considerable quantities of liquefied gases: *e.g.* SO_2: for exhibiting *quantitatively* the reactions on which the manufacture of sulphuric acid is based; and for demonstrating the law of Dulong and Petit. For descriptions of these, and for other experiments, reference must be made to the original paper.

THE HYPOTHESIS OF ACCELERATED DEVELOPMENT BY PRIMOGENITURE, AND ITS PLACE IN THE THEORY OF EVOLUTION[1]

II.

THE problem before which we are here placed may be formulated as follows:—How is it, that while the tendency to vary which obtains in all organised beings, and which forms one of the foundation stones of the theory of evolution, how is it that this tendency has exerted upon a number of living beings a so much less considerable influence than upon others, so that even in the present day numerous representatives are found of the most primitive animal groups which belong to the oldest known in the geological succession?

Still more, why are there certain genera which, since the Silurian period, appear to have undergone a stagnation in their development, in their advance towards higher differentiation, whereas within a much shorter period the whole of the living mammalian fauna has developed out of more primitive vertebrates and the important modifications have taken place among these mammalia which have finally led to the appearance of the elephant on the one hand, and of the shrews on the other?

In other words, can it be assumed that this tendency to vary could be totally and persistently neutralised by other causes amongst whole series of living beings during thousands of years, whereas during the same number of years this tendency, aided by natural selection, could lead other series of animals along roads where they have advanced with gigantic strides?

I need not remind you that this objection against the theory of

[1] By Prof. A. A. W. Hubrecht. Inaugural Address delivered in the University of Utrecht. September, 1882. Continued from page 281.

evolution, which has also been felt and combated by Darwin, was very often advanced against it, especially in the beginning. Cuvier had already reminded Lamarck that the absolute identity between the Egyptian animals, as they were embalmed three thousand years ago, with those inhabiting the same provinces in the present day, rendered untenable his ideas about gradual change and perfection of organic beings.

Huxley, to whose close reasoning powers and untiring readiness for battle the rapid progress of evolution is in a great measure due, has devoted several pages to the refutation of this objection. His argument runs as follows[1]:—

The two chief factors in the process of evolution are: the one the tendency to vary, the existence of which in all living forms may be proved by observation; the other, the influence of surrounding conditions, both upon the parent form and upon the variations which are evolved from it. Now, as often as the first factor makes itself felt, and modified forms take their origin out of a common parent form, it will depend entirely on the conditions which give rise to the struggle for existence, whether the variations which are produced shall survive and supplant the parent, or whether the parent form shall survive and supplant the variations. If the surrounding conditions are such that the parent-form is more competent to deal with them and flourish in them, than the derived forms, then, in the struggle for existence, the parent-form will maintain itself, and the derived forms will be exterminated. But if, on the contrary, the conditions are such as to be more favourable to a derived than to a parent-form, the parent-form will be extirpated, and the derived form will take its place. In the first place there will be no progression, no change of structure through any imaginable series of ages; in the second place there will be modification and change of form.

So far Huxley. No doubt but he has made us acquainted with a very reliable explanation of how the variation of any form of animals or plants may be retarded. The hypothesis of degeneration first formulated by Anton Dohrn, and afterwards warmly advocated by Ray Lankester, is no doubt of considerable importance for our comprehension of numerous lower stages of organisation in the animal and vegetable world, which may no longer be looked upon as parent-forms of more highly differentiated groups, but which, on the contrary, have in their lineage much more complicated ancestors than their own stage of organisation would appear to show. At first sight these degenerated animals show different points of similarity with animal forms, lower than those to which they are genetically allied.

So, for example, the Tunicata have for a long time been arranged amongst or close to the Mollusca, but lately-continued researches have evermore tended towards the conclusion that we have here before us the degenerate descendant of animals which had already attained the level of the lowest Vertebrates, but whose descendants, thanks to degeneration, have at present all the appearance of Invertebrates. In this way the number of lower animal types which may be looked upon as primitive, and whose persistence through geological periods gives rise to the questions as formulated above, is deceptively increased by forms, which we must remove from amongst them, and place in the vicinity of their more direct allies.

The process of degeneration is, however, confined within certain limits; it cannot do the same service towards the refutation of the objection here dealt with as can Huxley's argumentation above referred to, which is fully directed against the cardinal point, and the value of which I cannot estimate highly enough.

Still it appears to me that his explanation of the lengthened persistence of so many of the lower organised animals and plants can yet be supplemented by a new hypothesis.

To this I give the name of the hypothesis of accelerated development by primogeniture. If I have the advantage to lay it before you to-day, you will bear in mind that it has as yet only a preliminary shape, and that for its ultimate confirmation extensive researches will yet be required.

The fact is daily confirmed by continuous observation, that not only numerous vertebrates, but also very many invertebrates, can attain a very old age without the capacity for reproduction being essentially diminished. This is confirmed by the recently published researches of Weissmann[2] on the connection between the length of the reproductive period and the duration of life. We may fairly assume that all those animals attaining an old age leave issue which has been born at different periods—issue from a youthful age, which itself has again brought forth children and grandchildren, and issue from old age, which is on a level with the fourth or fifth generation of the first-born descendants. An example of old age combined with successful attempts towards reproduction is furnished by the well known sea-anemone, "Granny," which was captured in 1828 by Dalyell on the Scotch coast, and being still alive, last year gave birth to a certain number of young Actiniæ.

The large Tridacnas and the gigantic Cephalopods which have now and then been observed, must also have attained a considerable age; nothing authorises us to maintain that these have been infertile in all the later years of their lives. We need not stop to consider the higher groups: fishes, birds, and mammalia. They all contribute during a shorter or longer time towards the procreation of the species, and the considerable age which both fishes and birds are known to attain is the cause of a very considerable difference in age of the oldest and the youngest individuals of their own breeding. And so all of them will leave both first-born and last-born posterity. With the first-born this will in their turn be the case, so with *their* posterity, and so forth. Similarly the last-born, when they have attained maturity, will bring forth a series of descendants of very different ages; the last-born of the last-born being the final term of this series.

After centuries the effect will be this: From one pair of parents a large number of descendants will have sprung, a small number of these being the descendants in a direct line of the first-born of every successive generation; another small number being the descendants in a direct line of the last-born of every successive generation, whereas the remainder belong to intermediate stages. The first-born are separated from the primitive parent form by a number of generations, x, which is necessarily a considerable multiple of the number of generations y, which lies between the same parent form and their last-born descendants. Evidently the difference in age between the first-born descendant and his parents is a minimum, for the sole reason of his being the first-born, that between the last-born descendant and these same parents being on similar grounds a maximum. Thus, if we follow up in the direct line of descent the series of first-born of the first-born, &c., we find that the distance between two terms of that series corresponds to a much smaller number of years than the distance between two terms of the series of the continually last-born, which have always descended from last-born.

Comparing these two series simultaneously after the lapse of centuries, the series of the first-born will count numerous terms, many generations, at short distances from each other, whereas the series of the last-born will, on the contrary, consist of a much smaller number of terms, each of which is separated from its predecessor by a much more considerable distance. It is the number of these terms which in the one case I wished to express by x, in the other by y.[1]

From this fact we are led to propose the following question: Is there any reason to expect, that in the struggle for existence, the representatives of each of the two divergent series are collectively provided with different weapons? Or are both these groups quite equal to each other in the struggle?

Both observation and theoretical deduction force the conclusion upon us that a difference is indeed present. A difference, (1) in the external circumstances under which the first-born and the last-born come into existence; (2) in the internal properties and acquirements with which both series are provided; a difference which does not appear sporadically between certain representatives of both groups, but which may indeed be collectively observed between all of them.

As to the first point, the external circumstances, I call your attention to the following example, which shows how nature indeed makes a difference on a large scale in the conditions—under which she awaits the first-born and the last-born progeniture.

[1] American Addresses, p. 39.

[2] A. Weissmann, "Ueber die Dauer des Lebens," 1881.

[1] I am doubtful whether there are indeed *first-born descendants* in the pure signification of the word, *i.e.* such which, both from the paternal and from the maternal side, count only first-born in the whole of their ancestry. However, this does not materially influence our argument. We bring together in the series of first-born all those descendants in which mixture and intercrossing with second and third births was always reduced to a minimum, whereas on the other hand, in the group of the last-born, not only those cases which are theoretically pure are brought together, but those in which the number of ancestors on both sides most closely approaches to the number of generations y, which lies between the last born *in abstracto* and the common parent form. In the majority of cases, however, intercrossing and blending will have occurred on a large scale, and the average number of generations which leads from them to this parent form may be expressed by $\frac{x+y}{2}$. The calculus of probabilities would be able to furnish us in any given case, supposing enough data are available, with the exact grouping of these numbers.

From the observations which Livingston Stone has made in 1878 in the North American institutions for fish-culture on the McCloud River, it follows that 14,000,000 eggs obtained from ripe but relatively young and smaller salmon were without exception at least one third smaller than the millions of eggs which were before obtained from older, larger salmon of the same species, and that nevertheless they developed quite normally. By these observations the fact is established that the salmon, when older, lays larger eggs than at a more youthful age, and this, more especially, is of great value for our hypothesis. Firstly, the size of the egg must influence the chances which they have for escaping or falling a prey to different voracious animals. In this respect the smaller eggs are exposed to other dangers than the larger ones. Furthermore, the relative size of the egg will, without doubt, exert a certain influence—however insignificant—upon the individual which is developed out of it.

In comparison with the larger egg of the older salmon, either the food-yolk or the formative yolk in the smaller one will be of smaller dimensions, or both together will have been reduced in size. In each of these three cases, even in the last-named, the conditions under which the smaller egg (that is to say, the whole generation of the first-born) attains its development, differ from those of the generation issued from the larger eggs, the generation of the last-born. The first-born will either be of smaller size, or because they possess a smaller food-yolk they will have to provide their own nourishment at an earlier date; or both circumstances are combined.

Nobody will deny that in each of these cases natural selection can freely come into play. In addition to this it must be remarked that however insignificant this difference in external circumstances may be its presence is nevertheless undeniable, since it reappears again with unerring certainty in every successive generation. In this way the effect can gradually accumulate, and finally the path may have been entered upon which leads to a specific differentiation of the descendants of the first and of the last born.

This having taught us that indeed the external circumstances which preside at the birth and at the growth of the first and the last born are different (at least for this species of salmon, reliable observations on a similar scale concerning other animals being for the present wanting), I must now call your attention to the second cardinal point, viz., that the internal properties and acquirements with which each of the two series of births is provided, are also different. Heredity has indeed invested them with peculiarities, part of which show themselves in their organisation, another part remaining latent, and only attaining development in following generations. Such a latent potential tendency towards eventual modification of the individual or his progeny, must needs find more numerous occasions to unfold itself in the first born, simply because these are possessed of a larger number of ancestors. On the contrary those that have a smaller number of ancestors, *i.e.* the last born, have had this occasion for development offered to them at rarer intervals. From this it follows that further modifications under the influence of natural selection will be started by preference in the different series of first born, because *ceteris paribus*, there are here more chances for the appearance of small deviations, which to a certain extent are always due to reversion to the parent forms.

And so there is reason to suppose that also the internal properties of the series of first born differ from those of the last born, in the same way as we have just defined it for external agencies. In my opinion the difference in internal structure is of greater consequence than that in external agencies, although we must at the same time acknowledge that our present methods do not allow us to test this experimentally. Only by extensive and long-continued experiments more light will be thrown on this subject. The example which was mentioned of the seventy-years-old sea anemone, which reproduced itself successfully proves that the material for similar experiments is not deficient. In the vegetable kingdom forms will certainly be hit upon which will fully reward the difficulties of the experiment.

Once a new species, modified and generally higher-differentiated, having arisen out of the first-born by gradual accumulation of the small deviations, intercrossing and bastardising with the last-born descendants of the parent form, becomes rarer, copulation taking place by preference with specimens of the same species, and only exceptionally with representatives of the species which has lagged behind in its development. For this new species the same process sets in; here, too, the first born progeniture will surpass in the course of years the last-born, and will in its turn give rise to new modifications. And so *ad infinitum*.

We now come to another important point, which is in direct connection with the question, which are the last-, which the first-born. With most lower animals—Protozoa, Cœlenterata, Echinoderms, Worms—reproduction by fission is very common by the side of reproduction. Cut arms of starfishes grow to be complete starfishes after having passed the so-called "comet" stage; certain annelids divide themselves after one of the posterior body-segments have become converted into a head; certain Nemertines break themselves into pieces under spasmodic contractions, each fragment being able to reproduce both head and tail; Amœbæ divide themselves into halves.

Now it cannot be admitted that in fissiparous reproduction, heredity can come into play in the same measure as it can in the case of sexual reproduction. It is not even possible to determine which of the two halves represents the older generation. Weissmann has lately humorously said: if we fancy an Amœba gifted with consciousness, she will think upon dividing into two, "I now bring forth a child," and there is no doubt that each half would look upon the other as the child, and upon itself as the mother. Weissmann has thus introduced the idea of the (approximate) immortality of the Protozoa, an idea which can also be adduced in favour of the hypothesis here maintained, and which at all events deserves to be mentioned by the side of the hypothesis proclaimed by Hæckel and others, viz. that the Monera living in the present day are in no genetical connection with older ancestors from earlier periods, but have come into existence by the aid of repeated spontaneous generation.[1]

The same views hold good for the self-division of worms and Cœlenterata. Here too both parts are the direct continuation of a single individual which, although dividing, does not cease to exist. Coral reefs which principally multiply by division may be looked upon in the same way.

Never, in case of fissiparous reproduction, does that mysterious potentiation take place which brings together in the egg-cell and in the spermatazoön, not only the characteristic properties of father and mother, but of whole series of ancestors; never in this case can the special process of fixation of a part of these latent forces, the process which we term *heredity*, take place to its full extent. Never can selection during embryonic and larval life, which, according to recent researches, plays a much more conspicuous part than was originally expected, favour the stability of a variation, and thus lead to modification of the species, where multiplication by division takes place.

In his chapter on pangenesis ("Origin of Species," second edition, pp. 353 and 390) Darwin too touches upon this subject, and insists upon the fact that organisms produced asexually, consequently not passing through the earlier phases of development, "will therefore not be exposed *at that period of life when structure is most readily modified* to the various causes inducing variability in the same manner as are embryos and young larval forms."

The series of generations which owe their origin to a-sexual and not to sexual reproduction, are thus in a much lesser degree liable to vary.[2] And yet a variation of some sort must always first occur, in order that natural selection, acting upon it, may finally produce a definite modification of the species. Nevertheless, fissiparous multiplication continues to play—and has always played—a very important part in the invertebrate kingdom, *by the side* of sexual reproduction. Thus the presumption is allowed, that where in the course of centuries a-sexual reproduction has been more predominant than sexual reproduction, a stagnation in development has resulted, the differentiation of those series of individuals and genera which have originated through sexual reproduction, in the meantime always continuing its regular course onwards.

Both factors—the retardation of development by a-sexual reproduction, and the acceleration of the development of the always first-born, make it very probable, in my opinion, that we have to look upon the more highly-developed groups of animals, and amongst these upon their higher-differentiated representatives, as forms which are separated from the original parent stock by a maximal number of ancestors, the number of times that a-sexual

[1] Lamarck had already, by this same assumption, attempted to overcome the difficulty.

[2] Observation tends to confirm this in a general way (*vide* Darwin, *l.c.*, p. 353.

reproduction has taken place in their ancestry being at the same time reduced to a minimum.

On the contrary, we must expect that a much smaller number of ancestors lies between the lower-developed groups and the common parent form, that a-sexual reproduction has here more repeatedly occurred, and that finally, Darwin's and Huxley's explanation, which we have above alluded to, of the non-occurrence of further modifications, may here have been realised to a greater extent.

Keeping in view the combined action of both these principles, we no longer wonder that even in the present day living representatives are found of genera which were already present in the Silurian epoch, nor that the simplest organised beings have continued to exist in that primitive form.

They are for the greater part the younger sons, and being condemned to a slower rate of development, they could not keep apace of their elder brothers. The latter, which have so much oftener passed through the improving crucible of sexual reproduction, are indebted to that cause for having become the parent stock out of which the higher and highest-developed animal and vegetable forms, now surrounding us, have gradually sprung.

THE ETHER AND ITS FUNCTIONS[1]

I HOPE that no one has been misled by an error in the printing of the title of this lecture, viz. the omission of the definite article before the word ether, into supposing that I am going to discourse on chemistry and the latest anæsthetic; you will have understood, I hope, that "ether" meant *the* ether, and that the ether is the hypothetical medium which is supposed to fill otherwise empty space.

The idea of an ether is by no means a new one. As soon as a notion of the enormous extent of space had been grasped, by means of astronomical discoveries, the question presented itself to men's minds, what was in this space? was it full, or was it empty? and the question was differently answered by different metaphysicians. Some felt that a vacuum was so abhorrent a thing that it could not by any possibility exist anywhere, but that nature would not be satisfied unless space were perfectly full. Others, again, felt that *empty* space could hardly exist, that it would shrink up to nothing like a pricked bladder unless it were kept distended by something material. In other words, they made matter the condition of extension. On the other hand, it was contended that however objectionable the idea of empty space might be, yet emptiness was a necessity in order that bodies might have room to move; that, in fact, if all space were perfectly full of matter everything would be jammed together, and nothing like free attraction or free motion of bodies round one another could go on.

And indeed there are not wanting philosophers at the present day who still believe something of this same kind, who are satisfied to think of matter as consisting of detached small particles acting on one another with forces varying as some inverse power of the distance, and who, if they can account for a phenomenon by an action exerted across empty space, are content to go no further, nor seek the cause and nature of the action more closely.[2]

Now metaphysical arguments, in so far as they have any weight or validity whatever, are unconscious appeals to experience; a person endeavours to find out whether a certain condition of things is by him conceivable, and if it is not conceivable he has some *primâ facie* ground for asserting that it probably does not exist. I say he has *some* ground, but whether it be much or little depends partly on the nature of the thing thought of, whether it be fairly simple or highly complex, and partly on the range of the man's own mental development, whether his experience be wide or narrow.

If a highly-developed mind, or set of minds, find a doctrine about some comparatively simple and fundamental matter absolutely unthinkable, it is an evidence, and it is accepted as good evidence, that the unthinkable state of things is one that has no existence; the argument being that if it did exist, either it or something not wholly unlike it would have come within the range of experience. We have no further evidence than this for the statement that two straight lines cannot inclose a space, or that the three angles of a triangle are equal to two right angles.

[1] A lecture by Prof. Oliver Lodge at the London Institution, on December 28, 1882.

[2] In illustration of this statement an article has since appeared in the January number of the *Philosophical Magazine*, by Mr. Walter Browne.

Nevertheless there is nothing final about such an argument; all that the inconceivability of a thing really proves, or can prove, is that nothing like it has ever come within the thinker's experience; and this proves nothing as to the reality or non-reality of the thing, unless his experience of the same kind of things has been so extensive as to make it reasonably probable that if such a thing had existed it would not have been so completely overlooked.

The experience of a child or a dog, on ordinary scientific phenomena, therefore, is worth next to nothing; and as the experience of a dog is to ordinary science, so is the experience of the human race to some higher phenomena, of which they at present know nothing, and against the existence of which it is perfectly futile and presumptuous to bring forward arguments about their being inconceivable, as if they were likely to be anything else.

Now if there is one thing with which the human race has been more conversant from time immemorial than another, and concerning which more experience has been unconsciously accumulated than about almost anything else that can be mentioned, it is *the action of one body on another*; the exertion of force by one body upon another, the transfer of motion and energy from one body to another; any kind of effect, no matter what, which can be produced in one body by means of another, whether the bodies be animate or inanimate. The action of a man in felling a tree, in thrusting a spear, in drawing a bow; the action of the bow again on the arrow, of powder on a bullet, of a horse on a cart; and again, the action of the earth on the moon, or of a magnet on iron. Every activity of every kind that we are conscious of may be taken as an illustration of the action of one body on another.

Now I wish to appeal to this mass of experience, and to ask, is not the direct action of one body on another across empty space, and with no means of communication whatever, is not this absolutely unthinkable? We must not answer the question offhand, but must give it due consideration, and we shall find, I think, that wherever one body acts on another by obvious contact, we are satisfied and have a feeling that the phenomenon is simple and intelligible; but that whenever one body apparently acts on another at a distance, we are irresistibly impelled to look for the connecting medium.

If a marionette dances in obedience to a prompting hand above it, any intelligent child would feel for the wire, and if no wire or anything corresponding to it were discovered, would feel that there was something uncanny and magical about the whole thing. Ancient attempts at magic were indeed attempts to obtain results without the trouble of properly causing them, to build palaces by rubbing rings or lanterns, to remove mountains by a wish instead of with the spade and pickaxe, and generally to act on bodies without any real means of communication; and modern disbelief in magic is simply a statement of the conviction of mankind that all attempts in this direction have turned out failures, and that action at a distance is impossible.

If a man explained the action of a horse or a cart by saying that there was an attraction between them varying as some high direct power of the distance, he would not be saying other than the truth—the facts may be so expressed—but he would be felt to be giving a wretchedly lame explanation, and any one who simply pointed out the traces would be going much more to the root of the matter. Similarly with the attraction of a magnet for another magnetic pole. To say that there is an attraction as the inverse cube of the distance between them is true, but it is not the whole truth; and we should be obliged to any one who will point out the traces, for traces we feel sure there are. If any one tries to picture clearly to himself the action of one body on another without any medium of communication whatever, he must fail. A medium is instinctively looked for in most cases, and if not in all, as in falling weights or in magnetic attraction, it is only because custom has made us stupidly callous to the real nature of these forces.

When we see a vehicle bowling down-hill without any visible propelling force we ought to regard it with the same mixture of curiosity and wonder as the Chinaman felt when he saw for the first time in the streets of Philadelphia a tram-car driven by a rope buried in a pipe underground. The attachment to these cars comes through a narrow slit in the pipe, and is quite unobtrusive. After regarding the car with open-mouthed astonishment for some time, the Chinaman made use of the following memorable exclamation, "No pushee—No pullee—Go like mad!" He was a philosophic Chinaman.